AN
ESSAY
ON THE
VITALITY OF THE BLOOD.

ENTERED AT STATIONERS' HALL.

80 k

AN ESSAY ON THE VITALITY OF THE BLOOD.

BY

JAMES CORRIE, M. D.

K

"But fleſh with the life thereof, which is the blood thereof, ſhall
"you not eat." GENESIS, ch. ix. v. 4.

DR. CAIUS.—"Sir John, you vould do vell to loſe little blood.
FAL.—"Me! I thank thee. But in the blood is the life of the crea-
"ture; and I will not conſent to part with mine."
FALSTAFF's WEDDING, act iii. ſc. ii.

LONDON:
Printed for ELLIOT and KAY, No. 332, oppoſite Somerſet
Place, in the Strand; and W. CREECH, Edinburgh.
1791.

MVSEVM
BRITAN
NICVM

TO

JOHN HUNTER, Eſq. F. R. S.

Surgeon Extraordinary to His Majeſty, Surgeon General to the Army, &c.

SIR,

As ſoon as I reſolved to prepare a Theſis, the Vitality of the Blood was the ſubject moſt forcibly impreſſed on my mind for that purpoſe; but at that time it had ſcarcely entered my imagination that I ſhould ever have an opportunity of ſeeing or knowing the celebrated author of that opinion perſonally.

But ever ſince I commenced the

ſtudy of medicine, my intentions have been to ſet ſuch an eminent pattern before my eyes, and to endeavour to follow, though at a great diſtance, ſuch a praiſe-worthy example; and whenever I determined to publiſh, in its preſent form, the following little Eſſay, I reſolved, and I think with propriety, to lay it before you, to whom I owe the firſt and leading principles thereof; ſenſible at the ſame time, that no dedication whatever can add to your fame, it ſtands on a much more ſtable foundation.

I am alſo well convinced, that I never could ſubmit this juvenile performance to a more capable or more impartial judge; and whereever I have been led into any error or miſtake, (for miſtakes, no doubt, there are) I ſhall be happy to be corrected; and where it may appear I have followed the right path, if it merits your approbation, it will give the utmoſt pleaſure to,

SIR,

Your moſt obedient,

Moſt humble,

And devoted ſervant,

JAMES CORRIE.

LONDON, MAY 25, 1791.

AN
ESSAY
ON THE
VITALITY OF THE BLOOD.

IN compoſing the following eſſay I had ſeveral incitements; but diſappointed in ſome of them, I have, with the advice of a few friends, ventured to lay my thoughts on this ſubject before the public in the preſent form; not ſuppoſing that much new matter will be found therein, but that the attention of men of learning and experience, may be drawn to a ſubject undeſervedly buried in oblivion; for while it was univerſally allowed that the animal machine, although acknowledging the influence of the common laws of matter, was governed by a principle peculiar to

life,

life, yet this principle was never till lately ſuppoſed a property of the blood of animals: for although in the works of the immortal *Harvey*, we find ſome looſe hints which may lead to the concluſion, that he ſuppoſed the blood of animals poſſeſſed of a living power, yet the doctrine, it would ſeem, had made few converts, and was, I may ſay, entirely diſcarded, till that truly able philoſopher Mr. John Hunter of London, called the attention of phyſicians to it not many years ago; ſince which, ſome have laboured not a little to combat the opinion, while another as ſtrenuouſly defends it, even from ſacred writ.

How far my experiments and reaſoning corroborate the opinion, does not become me to judge; with diffidence I preſent them to the public, and claim that indulgence which is due to a very young man.

As moſt of the experiments were unavoidably made on a ſmall ſcale, the reſult may, in many caſes, when repeated on a larger, be ſomewhat different; yet I preſume they will be found ſufficiently accurate for aſcertaining the end I had in view. No doubt inaccuracies may have crept in, and that either from accident, or my thinking I ſaw that which I wiſhed to ſee; which laſt is often the caſe with thoſe who feel a warm affection for a child of their own fancy.

In ſeveral places I have purpoſely avoided recommending practices which reſult as a conſequence of the doctrine inculcated; leaving that to men of greater experience and depth of judgement; and upon the whole, although we have failed of producing a clear conviction, yet from the colliſion of facts, ſparks I hope are thrown out ſufficient to ſhew a future adventurer his way.

Having neceſſarily, as I think, premiſed this much, I proceed to ſtate the order in which I mean to treat of this ſubject; and that which at preſent appears to me the beſt will be:

1ſt. To ſhew that fluids, as well as ſolids, can admit of animation.

2d. To define what I underſtand by the animation or life of the blood.

3d. To prove that, in the ſenſe adopted, life exiſts in the blood.

4th. And laſtly, endeavour to determine the ſource from whence the blood derives its life.

To prove the firſt poſition, naturally occurred as the firſt ſtep of this enquiry; ſince ſome have taken upon them to aſſert, that in even the nature of things, blood cannot be alive; becauſe, being fluid, it does not admit of organization; life, as they ſay, conſiſting in the performing of certain functions of an active kind; and for the performance of theſe functions, organization they view as abſolutely neceſſary [*].

In a different light, however, it appears to me; for the only real difference betwixt a ſolid and a fluid ſeems to conſiſt, in the particles of the latter being more moveable the one upon the other; we are alſo accuſtomed to view different bodies in both ſtates, and from theſe alone, may we not affirm, "that there is not a more intimate "connection between life and a ſolid, than "life and a fluid."

---

[*] Dr. Hendy's Eſſay on Glandular Secretion, p. 70.

To confirm that, however, we can go farther, and as the works of nature are very obſcure, I do not ſee why a certain obſcure organization may not ſubſiſt in the blood of animals, that does not come within the reach of the evidence of our ſenſes, and even although in that ſtate, not the leſs real; for would any perſon take upon them to deny, the organization of the embryo, in its early ſtate, becauſe it is not obvious to our ſenſes.

Farther it appears to me, that what Dr. Hendy defines life to be, is only a conſequence of life, conjoined with a certain organization, and that from either a want of organization, or difference thereof, the effects this life ſhew, acted upon by the ſame cauſe will be different; and therefore although we do not find the ſame phenomena follow, from the ſame cauſe acting upon muſcular fibre and blood, we are not at liberty to ſuppoſe the blood dead, ſince different phenomena evidently reſult, from the ſame ſtimulus acting upon evidently organiſed living matter.

But although different phenomena reſult, from the application of the ſame cauſe to organiſed living matter, yet one general law affecting all living bodies is, that they have a tendency to reſiſt the action of thoſe bodies which threaten their deſtruction. This is undoubtedly the effect of life in every caſe; and hence, if we can find an animal ſubſtance devoid of apparent organization, poſſeſſed of ſuch a power, we are irreſiſtibly led to infer, that the blood may, for any thing we know to the contrary, be poſſeſſed of life.

That there are animal ſubſtances, devoid both of apparent organization and ſpontaneous motion, poſſeſſed of a power of reſiſting the action of hurtful bodies, every one will be perfectly convinced of, when he conſiders even the phenomena of incubation.

Experiments have ſhewn that the egg, which comes undoubtedly under the deſcription, is poſſeſſed of a conſiderable power, both of reſiſting heat, cold, and putrefaction, and that too in a degree nothing inferior to many of the leſs-perfect animals.

To prove the firſt part of this aſſertion, that the egg, although deprived of viſible organization and ſpontaneous motion, is capable of reſiſting, for a length of time, heat, we have only to mention what happens during the proceſs of incubation, where in that of the hen, it is ſituated in a heat of 103 deg. F. ſcale for three weeks, and in the ſame heat in the ducks for four; yet the yolk, which is not diminiſhed, is always perfectly ſweet to the very laſt; alſo that part of the albumen which is not expended on the growth of the animal, ſome days before hatching is alſo perfectly ſweet. But if the egg is not hatched, it becomes as certainly putrid, in very near the ſame time, as any other dead animal matter.

But eggs ſtand more direct proofs of a living principle, by reſiſting of cold, the ſecond aſſertion. Various experiments of Mr. Hunter's confirm this*; all of which I have had the curioſity to repeat, and found to correſpond ſo, that I could eaſily account for all the difference, which was ſmall, from a difference in the thermometer. For a full account of them, I refer to Mr. Hunter's work itſelf: here I will barely mention, that from them we find, that by freezing an egg, thawing it, and then placing it in a cold about (O) (being that temperature in which it was formerly frozen) along with another new-laid egg, we find that the former freezes nearly ten minutes ſooner than the latter.

---

* Hunter's Animal Oeconomy, p. 106.

We alſo find that an egg froze, in an atmoſphere fluctuating between 15° and 17° deg. above (O) then thawed and expoſed in an atmoſphere below freezing, yet nearly ten degrees warmer than that in which it was formerly froze, will freeze in one half the time it did before.

And finally we find, that when an egg, which has been once frozen and thawed again, and a new-laid egg, are put into a cold at or about 15° above (O), that the thawed one ſoon comes to 32°, begins to ſwell and congeal; while the freſh one ſuffers its heat to be diminiſhed to even 29° or 30°, and remains in that ſtate for nearly twenty minutes; after which it ſuddenly riſes to 32°, the freezing point, and begins to ſwell and congeal.

That theſe few ſimple experiments prove our poſition cannot but be obvious to every obſerver; for unleſs there were attached to the fluids of the egg a principle of life, how could it reſiſt the action of ſuch a heat for ſo long a time, as, during the proceſs of incubation, this property being only peculiar to living bodies; for when this principle is deſtroyed in the egg, and ſituated under ſimilar circumſtances, it becomes putrid nearly as ſoon as any other dead animal matter; and why not as ſoon as other dead animal matter, is, perhaps, owing to the leſs free action of the air. The other three ſets of experiments taken conjunctly, prove that cold, when applied in certain degrees to eggs, has a power of deſtroying ſome principle which has a tendency to reſiſt the action of ſaid cold: hence life; for after its deſtruction by a freezing proceſs, they no longer reſiſt the cold, but freeze in one half the time they did before, which certainly would never take place, were not ſome principle deſtroyed or expended in the former freezing proceſs, while endeavouring to reſiſt that which threatened its deſtruction.

Having thus, then, determined that the egg, an animal ſubſtance, evidently poſſeſſing neither viſible organization nor ſpontaneous motion, is poſſeſſed of a principle of life; farther proof is not neceſſary to confirm the idea that fluids can admit of a principle of life; yet we can carry the point farther, and, as I ſaid, clearly ſhew that theſe ſubſtances poſſeſs this principle, in a degree nothing inferior to many of the leſs perfect animals.

That this is true, a ſet of experiments, no leſs happily inſtituted than judiciouſly executed by Mr. Hunter, fully confirm *; but for particulars I refer to the work itſelf; here I will only mention in the moſt curſory manner, that having placed vipers, frogs, eels, ſnails, and leeches, in a degree of cold ſimilar to what was uſed in the experiments related on eggs, that they ſuffered their heat to be diminiſhed below the freezing point ſimilar to the egg, and having continued in that ſituation for nearly the ſame length of time, they generally then roſe to 32°, the freezing point, and congealed; by which

---

* A. Oecon. p. 104.

proceſs, after they were thawed, they were generally found to be dead, more eſpecially in the ſnails and leeches.

Here, then, we find that the ſame cauſe, applied in ſimilar circumſtances to animals, as well as animal ſubſtances devoid of apparent organization and ſpontaneous motion, produce the ſame effect; hence we cannot but infer, that the principle which cauſes both is the ſame. The ſame effects are alſo ſhewn from the application of another cauſe; for while, as experiments ſhew*, vipers and frogs poſſeſs a conſiderable power in reſiſting heat, yet they evidently have it not in a greater degree than the egg; while this laſt, as would appear by another experiment, evidently has a power in this point ſuperior to animals not the leaſt imperfect; for it appears that upon putting a living and a dead eel, and a living and dead tench into warm water, all of them receive that heat alike quick; and when again expoſed to the cold, part all of them alike quick with it; while by experiment it is found that an egg, when placed in warm water, reſiſts the heat for ſome length of time, and more particularly when new laid; and I have been informed by an experienced cook, that a new-laid egg requires conſiderable more time to boil, than one which has been laid for ſome time, which I could only explain by ſuppoſing the power of reſiſtance greater, and conſequently the principle of life more vigorous.

---

* Hunter's Animal Oeconomy, p. 105.

Theſe facts and obſervations, then, of Mr. John Hunter's, teach us, that animal ſubſtances, devoid of apparent organization and ſpontaneous motion, are poſſeſſed of powers capable of reſiſting heat and cold, nothing inferior to many of the leſs perfect animals, and as a natural conſequence of the former putrefaction alſo; for it is only by animals, or animal ſubſtances, being poſſeſſed of life, that they reſiſt, when placed in certain ſituations, putrefaction. Hence, when we find that this power, which reſiſts the putrefaction, is as eaſily deſtroyed by heat or cold, in ſome animals, as animal ſubſtances devoid of apparent organization and ſpontaneous motion, we can but infer that the one reſiſts putrefaction as powerfully as the other, ſince the cauſe which enables them to reſiſt this putrefaction is alike eaſily deſtroyed in both.

Having thus, then, controverted the general law of Dr. Hendy, and ſhewn that fluids can poſſeſs a principle of life, and hence that the blood, for any thing we know to the contrary, may poſſeſs this principle, we come to the ſecond great head, the defining what I underſtand by the life of the blood: previous to which, however, it is neceſſary to remark, that we are entirely ignorant of what this principle in ſubſtance conſiſts of; it is only from its effects we can judge of its preſence; for while attached to matter, that matter, beſides acknowledging the influence of the common laws of matter, is governed by a principle peculiar to life. Hence our definition will not conſiſt of what life is, only the effects which this life ſhew when acted upon by other agents.

The definition which follows is the beſt of any which has as yet occurred to me; I am far from thinking it perfect;

future obſervation may, perhaps, lead to this; at preſent for want, we content ourſelves with it, conſcious, however, that if we prove every part of it, imperfect, perhaps, as it is, we infallibly eſtabliſh a living principle in the blood.

DEFINITION.

By life, then, in the blood, I underſtand that property in it, by which, while attached to it, it is rendered incapable, when extravaſated, of acting on the living ſolid as dead matter; by which it is ſenſible to the application of a ſtimulus; by which it reſiſts the action of thoſe cauſes which tend to its deſtruction; and that principle which is the cauſe of all the actions that it induces.

This definition is naturally divided into four ſections, each of which I will briefly endeavour to prove, which leads to the third great head I have marked out, and to which I ſhall immediately proceed; and on the proving of which depends the truth of the whole doctrine.

That the blood, then, is poſſeſſed of a

property by which, while attached to it, it is rendered incapable, when extravaſated, of acting on the living ſolids as dead matter, I will endeavour to prove by ſhewing, that from its application to them, the ſame effects do not follow as follow that of dead matter. Here in this part we are led to enquire, whether the effects which the blood ſhew be different when contained in its proper veſſels, or when extravaſated, and conſequently unconnected with the ſource of life. To me, in both caſes, it appears the effects are the ſame, ſo long as this principle remains attached to the blood; but as the conſideration of the effects of the blood, when flowing in its proper veſſels, is more properly comprehended under the fourth head, we wave the enquiry as to it, and only endeavour to determine what the effects are of the blood extravaſated, and applied to the animal living ſolid; previous to which we will mention what the effects are which follow the application of dead or inanimate matter, by which we ſhall be able to aſcertain if the blood can be comprehended under that claſs.

The effects which follow the application of dead matter to the animal living ſolid are ſo well and univerſally underſtood, that it is ſcarcely neceſſary to inveſtigate it very particularly, it being evident, that by their action a particular proceſs is brought about, which either terminates in the throwing off the dead part, or, if ſuch part was previouſly inſerted betwixt the lips of a wound, ſo that nature could not effect its removal; ſo long as it remains in that ſituation no union of parts will follow. Theſe are the conſtant and invariable occurrences which follow the action of dead matter on living bodies; but the action of the blood is different; conſequently we can but infer that blood is governed, in this particular, by laws different from thoſe of inanimate matter, which were it not the caſe, as blood is commonly, in a greater or leſs degree, inſerted betwixt the lips of every wound, no union of parts could take place in the animal kingdom. Agreeable to this we find that one of Mr. Hunter's arguments is *, "that it unites living parts in ſome cir-

---

* Ed. Medical Commentaries.

"cumſtances, as certainly as the yet recent juice of the branch of one tree "unites it with that of another:" and, ſays he, "was either of theſe to be con"ſidered as extraneous or dead matter, "they would act as ſtimuli, and no union "would take place either in the vegetable "or animal kingdom."

The truth of this argument is, from what has already been ſaid, ſufficiently evident; yet Mr. Hunter, to render it ſtill, if poſſible, free from objection, ſtates an experiment made on the teſticle of a cock introduced into the abdomen of a hen; the former of which, while bloody, he introduced into the abdomen of the latter: upon killing the hen ſome time after, and injecting her, he found that he injected the teſticle alſo, which had adhered to the liver. Dr. Hendy, in his Eſſay on Glandular Secretion, (a work replete with much uſeful information), and his follower, Dr. Adye, in his inaugural Diſſertation de Sanguinis Circuitu, take upon them to combat the idea of life in the blood as introduced by Mr. Hunter. Their reaſoning, however, will in many places, I truſt, be found unphiloſophical,

and

and not ſtrictly juſt: but as Dr. Hendy was the firſt to impugn Mr. Hunter's doctrine, and has brought almoſt all the arguments himſelf againſt it, his follower only repeating them, we will direct our chief attention to him; and here we ſhall find that thoſe objections, which he brings againſt this argument, is a mere ſophiſm. Mr. Hunter having uſed the word extraneous, Dr. Hendy waving the argument, ſenſible of its force, catching at words, merely propoſes (in my opinion to the end that he may divert the attention) the following queſtion: "Why did not the teſticle of the "cock act as dead matter, when it came "in contact with the liver, and produce a "particular proceſs, according to Mr. "Hunter's own idea, which would terminate in the throwing off the dead or *ex-"traneous* part from the living or ſound "parts?" Hendy here alſo waves the enquiry, but not with juſtice, whether the teſticle was alive or no, merely ſaying, that it was certainly extraneous as to the liver of the hen, and ought, according to Mr. Hunter's own idea, not to have adhered thereto. Dr. Hendy's explanation of the word extraneous is, perhaps, here literally juſt; but then it is evident that Mr. Hunter uſes the word extraneous to expreſs a matter foreign to animal life, and evidently devoid of animation; while this is the caſe then, and while the effects are conſtant from the application of ſuch a cauſe, no ſuch effects following the action of the teſticle beſmeared with blood, we are irreſiſtibly led to the concluſion that both, in every ſenſe of the word, are poſſeſſed of life. That the former was alive, although deprived of ſenſation, is evident from what we ſee happen to it when either ſtimuli, chemical or mechanical, are applied, and alſo from analogy, where we find that other parts cut out of animals can be acted upon by ſtimuli, even for a length of time, and in ſome caſes even the ſeparated part has the power of becoming an animal perfect in all its parts; and, as we ſaid before, we cannot but admit the blood to be alive, ſince it has not the effects of other dead matter; it is, indeed, impoſſible to conceive that were it dead, veſſels could ſhoot through it, for it would be as effectual a bar to their elongation as a piece of bougie, or any other inanimate matter, which we often ſee uſed for that purpoſe: moreover, if I may be allowed the expreſſion, ſo far from conſidering it dead matter, I view it as the vinculum, uniting all thoſe parts which adhere by the firſt intention, as ſurgeons ſpeak, and, as ſuch, we cannot but admire provident nature; for by its inſinuation betwixt parts divided it effectually excludes air, than which nothing is more powerful in producing violent inflammation, and all its conſequences, by which the healing of wounds would be very much protracted in many caſes. In ſuch a ſituation blood, beſides anſwering this uſeful purpoſe, evidently appears from experiment poſſeſſed of another property; for while it thus glues, as it were, parts, and effectually excludes the introduction of inanimate matter, it beſides poſſeſſes a power of generating veſſels, and thus rendering the adheſion perfect and complete, which at firſt was only temporary, and liable from ſlight accident to a diſunion. This leads to the conſideration of another of Mr. Hunter's arguments, which is, "that blood "becomes vaſcular like other living parts, "for the coagula in the extremities of arte-

"ries, after amputation, may be injected "by injecting theſe arteries." Dr. Hendy admitting the verity of this argument, yet affirms, "that although this be the caſe, "nevertheleſs the fluids which theſe veſ-"ſels carry may not be alive; for (ſays he) "as the ſtump inflames, and as we have "ſhewn inflammation diſpoſes the veſſels "to elongate, by this they are elongated, "and paſs through the coagulum, and "anaſtomoſe; but they by no means "take their origin from the coagulum."

To theſe arguments of Dr. Hendy's, although ſpacious, I would only obſerve, that admitting inflammation diſpoſes the veſſels to elongate, how, as I have already ſaid, can we ſuppoſe them to paſs through dead matter; for if inſtead of the coagulum of blood we inſert a ſubſtance, to appearance the moſt analogous, the coagulum of wheat, into the mouth of an artery, we ſhall, to be ſure, find inflammation follow; but we ſhall never be able to inject veſſels ſhooting through this dead body; on the contrary, the inflammation induced would occaſion ſuch pain and conſequences as we ſee follow the ſhooting of inflamed veſſels into the interſtices of ſponge, when uſed to ſtop hemorrhage. Beſides Mr. Hunter has, he informs us, a preparation in which he can demonſtrate veſſels riſing from the center of a coagulum of blood, which entirely invalidates the laſt part of Dr. Hendy's objection, and tends much to confirm that, in the experiment on the hen, veſſels did ariſe from the coagulated blood. Moreover, what to me tends to confirm the opinion not a little is, that in thoſe caſes of wounds which heal by the firſt intention, a very ſlight degree of inflammation occurs, even in ſome caſes ſo ſlight as to be almoſt imperceptible; hence then, unleſs we ſuppoſe in the blood a tendency to generate new veſſels, how in ſuch a caſe does adheſion take place, ſince we find the only power (as they ſay) which unites parts is wanting.

Notwithſtanding theſe ſtrong proofs, Dr. Hendy even denies the poſſibility of veſſels taking their origin in coagula of blood, otherwiſe he would expect to find them in thoſe clots that are termed falſe conceptions, but which is by no means the caſe. To this I would ſay, that although veſſels have never been demonſtrated in ſuch clots, it is no proof that they do not exiſt: but ſetting aſide this, I think I may affirm that theſe clots are poſſeſſed of a certain degree of animation, otherwiſe how could they reſiſt the action of ſuch a degree of heat conjoined with moiſture without becoming ſoon putrid; therefore they muſt be poſſeſſed of a certain degree of life, as living bodies alone have this power; for we ſee that when a child dies in utero, which is by no means a rare occurrence, it becomes ſoon putrid: hence were theſe clots dead matter, they ought, being placed in ſimilar circumſtances alſo, to become putrid.

Here Dr. Hendy, finding what he thinks ſome ſeeming inconſiſtencies in Mr. Hunter's arguments, with an air of importance endeavours to turn them againſt him. I will attempt to ſhew, however, but with a becoming modeſty, that he impugns a doctrine, without being fully maſter of the arguments; for, ſays he, "blood "in its coagulated ſtate cannot poſſibly be "ſaid to poſſeſs life, according to Mr. "Hunter's own idea of life, who, in con-

" ſidering the blood as alive, views it as a
" fluid, as is evinced by his alledging, that
" in the nature of things there is not a more
" intimate connection between life and a
" ſolid, than life and a fluid." To which
he adds, " as the blood by coagulation loſes
" its fluidity and becomes ſolid, and as Mr.
" Hunter ſeems to admit that this coagula-
" tion is a morbid change produced by the
" ſtimulus of expoſure," hence he concludes, " admitting that the blood whilſt
" fluid was really alive, that it muſt, ac-
" cording to Mr. Hunter's own words, be
" conſidered, when in its coagulated ſtate,
" as being really and certainly dead."

To theſe arguments, in the firſt place, I would obſerve, that although Mr. Hunter ſays, that there is not a more intimate connection between life and a ſolid than life and a fluid, he does not ſay that the blood loſes life when it loſes its fluidity; on the contrary, he affirms, " we often ſee the
" ſame body fluid in one caſe, and ſolid in
" another:"* and certainly all that can be drawn from his arguments is, that the

---

* Med. Com. vol. II. p. 199.

life of the blood, in its fluid ſtate, is evinced by its being acted upon by the ſtimulus of air; and its life, in its ſolid or coagulated ſtate, is evinced by its not acting on the living ſolid as dead matter, and from its becoming vaſcular. Mr. Hunter neither, as I think, ſuppoſes coagulation a morbid ſtate, or from his words can it be inferred.

Dr. Hendy, however, intent upon refuting the opinion, makes Mr. Hunter to ſay ſo; and alſo in the next paragraph, what cannot poſſibly be inferred but from imagination alone. Mr. Hunter, he ſays, conſiders the life of the blood in the following ſenſe: "He conſiders a muſcle cut out of the body to be alive, as long as it "continues capable of being acted upon by "a ſtimulus of any kind; that is, as long "as any part of it retains its irritability." To which he adds, "Mr. Hunter agrees to "this, by ſaying, that the muſcles of a "turtle continue alive a great while after "the animal as one whole is dead." He having then fixed theſe as data of Mr. Hunter's, and as if at once to refute the poſſibility of life exiſting in the blood, propoſes the following as a query: "Does the coagu-

"lum poſſeſs the leaſt degree of this irrita-
"bility or life? as Mr. Hunter (he ſays)
"is pleaſed to term it." To which he
immediately anſwers: "No; it has cer-
"tainly loſt its irritability; and therefore
"(ſays he) Mr. Hunter himſelf not only
"agrees that coagulation is a morbid
"change, but by his own arguments
"proves that the coagulum of blood is
"a dead matter."

To theſe arguments, ſeemingly ſpacious, I would only obſerve, that having attentively conſidered all of Mr. Hunter's arguments, it was impoſſible for me to draw ſuch inferences as Dr. Hendy has done; for it evidently appears to me that Mr. Hunter uſes the analogy of irritability in muſcular fibre, not with the view of ſhewing that the blood poſſeſſes a life exactly analogous thereto, and that from the application of a ſtimulus ſimilar conſequences will follow; (this cannot be expected, owing to the evident difference of organization; by which we readily account for the different effects which follow the application of the ſame cauſe,) but merely with the intention of demonſtrating, by a familiar inſtance, that blood, although ſeparated or unconnected with the ſource of life, may yet, like muſcular fibre, retain this life for a length of time, although unconnected with the ſource of life. Hence we cannot but draw a different concluſion, the data being thus changed, and inſtead of ſaying with Dr. Hendy that we muſt infer, from Mr. Hunter's own arguments, that the coagulum of blood is really dead matter, we ſay, that the coagulation is a conſequence of the previous application of a ſtimulus to the blood when fluid, which had the effect of producing it without deſtroying, or even diminiſhing, the vital principle.

Theſe arguments then, to me, ſufficiently prove that the blood, when extravaſated, does not act on the animal-living ſolids as a dead, but living matter; conſequently were we in this place to enquire what its effects were when circulating in its proper veſſels, we ſhould be irreſiſtably led to the concluſion, that theſe effects muſt be a conſequence of its living power. This and the former are farther proved by arguments to follow under another head, and which demonſtrate directly the living power of the blood. Waving farther enquiry then, as to this head, we proceed to the ſecond, and endeavour to prove that blood is capable of being acted upon by a ſtimulus.

Senſibility to a ſtimulus is only the property of living bodies; conſequently, if we find the blood poſſeſſed of this power, we cannot but conclude it alive. Mr. Hunter, ſenſible of this, advances the following, and what, I think, true propoſition; "Blood," ſays he, "is capable of being "acted upon by a ſtimulus, for it coagu"lates on expoſure, as certainly as the ca"vity of the thorax or abdomen inflame "from the ſame cauſe." As I have already hinted the truth of this poſition, and the neceſſity of its being admitted, a ſlight digreſſion, if it ſhould occur, will, perhaps, be readily pardoned. Dr. Hendy*, however, thinks, with ſome degree of confidence, that he refutes it by merely mentioning, in the moſt ſceptical manner—"Any perſon would be laughed at, were "he to affirm that a jelly of any kind "was alive, becauſe in a certain heat it

---

[*] Eſſ. Gl. Sec. p. 67.

" continues fluid, and on a diminution of " this heat, or on expoſure to cold and " air, it coagulates."

Had I not been poſſeſſed of, perhaps, a more than common power over my riſible muſcles, I certainly, upon reading this, could not have refrained from laughing, to find a phyſician of Hendy's evident erudition treat in ſuch a manner ſo evident a forcible argument; but certainly ignorance of this laſt could only have led him to do ſo, unleſs, deſtitute of an argument to invalidate it, he had, as a dernier reſource, recourſe to turning it into ridicule: when ſuch arguments as theſe are uſed, it is a ſure proof to me of the want of others more forcible; for by this argument, when ſeriouſly conſidered, it can be ſhewn that there exiſts in the blood a principle which can be nothing but living, which is as eaſily acted upon by air, and peculiar conſequences reſult therefrom, as reſult from the action of ſtimuli, either mechanical or chemical, on parts poſſeſſed of irritability, although detached from the animal.

Beſides, had Dr. Hendy ſeriouſly conſidered the matter, he would never have drawn any analogy betwixt the coagulation of blood and that of jellies, it being evident to every obſerver, that the coagulation of the latter depends upon a diminution of its heat, and by an increaſe of this heat can again be rendered fluid, while that of the former depends upon no ſuch cauſe; and when once coagulated, cannot eaſily again be rendered fluid. It alſo evidently coagulates in ſome parts of the ſyſtem, when its heat is as great as when circulated in its proper veſſels.

But to entirely ſubvert this futile argument of Dr. Hendy's, (nothing leſs than futile can I call it), we need only to have recourſe to thoſe experiments of Mr. Hewſon's on the blood, which brought to light many important facts; there it is pointed out in the cleareſt manner, that the coagulation of blood does not depend either upon a diminution of its heat, or want of motion, but upon the action of air, although when applied it be as warm as the blood itſelf*.

---

* Hewſon's Experimental Enquiry, Exper. 6. p. 20.

This certainly, and without doubt, deſtroys the analogical argument of Dr. Hendy; for although we expoſe jellies of any denomination to the action of air alone, yet if we do not diminiſh that heat on which their fluidity depends, they will not coagulate. Hence it is evident their coagulation depends upon a diminution of that heat to which they owe their fluidity, while the coagulation of blood depends upon the action of air on its vital principle, and not upon a diminution of its heat.

To ſhew which in a ſtill clearer point of view, if need be, recourſe again to Mr. Hewſon's Experiments will ſufficiently demonſtrate that cold, when applied to blood detached from the ſyſtem, but included in its proper veſſel, ſo as to render the acceſs of air inadmiſſible, has, when continued for a length of time, although many degrees above freezing, the power of leſſening conſiderably the diſpoſition of that blood to coagulate, inſtead of coagulating it, which ought to occur were Hendy's analogy juſt; and what confirms this the more is, that blood can be even froze without coagulation taking place, and even after being thawed, by the action of air coagulate; and finally we find, by Exp. 26th of Hewſon, that blood included in its proper veſſel, and immerſed in water at 38 deg. above (O), and ſuffered to remain there for nearly twenty-four hours, upon taking it out and opening the veſſel the blood was found to be fluid, though viſcid; and although it was placed upon the window of a moderately warm room, and examined carefully from time to time, it never had any appearance of coagulation; on the contrary, it remained fluid till it was dried by the evaporation of the water. This, without the leaſt room for doubt, demonſtrates that the coagulation of blood does not, as jellies do, depend upon a diminution of that heat, obvious when in a fluid ſtate.

In addition to theſe arguments, Dr. Adye *, with a view to controvert the opinion, adduces the following: "Quod "multæ res inanimatæ eodem modo afficiuntur; albumen, exempli gratia, ovi, "quod nemo vivum eſſe exiſtimabit."

---

* Diſſ. Inag. de Sang. Circuitu. p. 88.

This argument of his, even ſuppoſing the albumen dead matter, I would object to, and ſay the analogy was not juſt. Will the albumen coagulate upon mere expoſure to air? No, it will not; then, as we have ſhewn this the power which coagulates blood ſooneſt, and has no effect on the albumen ovi, the analogy does not hold good, although both ſubſtances coagulate upon an increaſe of their heat; and even then I cannot ſuppoſe that this occurs before all the life which both ſubſtances poſſeſs is deſtroyed, provided the external air has not acceſs to the blood.

And to conclude, Dr. Adye advances that he cannot ſuppoſe it alive, or capable of being acted upon by a ſtimulus, becauſe it neither ſhews any ſigns thereof when the more acrid ſtimuli are applied; nor does alternate relaxations and contractions take place, which is what we ſee happen when the ſolids are acted upon by ſtimuli, either mechanical or chemical.

To theſe arguments I would object, and ſay, that although from the action of ſtimuli, on the irritability of muſcular fibre, relaxation and contraction alternate, yet it does not follow that the ſame effects ſhould take place when ſtimuli are applied to the blood; the difference of effect ariſing not from any difference in the life, but entirely from the different organization of the muſcular fibre; for to this laſt is entirely owing the phenomena of irritability, that is, a certain organization conjoined with life; which being the caſe, we cannot reaſonably ſuppoſe, the organization being wanting in the blood, either abſolutely, or as to kind, that the ſame phenomena could occur from the application of the ſame ſtimulus.

The ſame arguments alſo ſufficiently refute what Dr. Adye infers from the different action of common ſalt on the muſcular fibre and the blood; for evidently when applied to the blood, as ſingular conſequences follow, as when applied to the irritability of a muſcle; and to conclude this part, we might as well ſay that a muſcle did not poſſeſs life, becauſe we ſee that certain powers, when applied in a great degree, have the property immediately, and without any obvious effect, of entirely deſtroying the irritability or life of the fibre, ſo that it can be no longer acted upon by any ſtimulus; as that the blood has no life, becauſe common ſalt, and many others when applied thereto, have the property of rendering it, when undiluted, incapable of being acted upon by the ſtimulus of expoſure.

Having thus, I think, clearly eſtabliſhed Mr. Hunter's poſition, that there is a principle in the blood, by the action of air upon which the coagulation thereof depends, and ſatisfactorily invalidated all thoſe arguments which have been brought againſt it, it will nevertheleſs ſtill tend to render it more unexceptionable if we ſhew that it acknowledges the ſame laws as other living bodies, and that at different times it is variouſly affected from the different ſtate of life at the time, which leads us to the conſideration of another of Mr. Hunter's arguments, which is, "that the more it is
" alive, that is, the more the animal is in
" health, the ſooner it coagulates on ex-
" poſure; and the more it has loſt of its
" living principle, as in the caſe of vio-
" lent inflammation, the leſs it is ſenſible
" to the ſtimulus produced from its being
" expoſed, and the later it coagulates."

The

The verity of this obſervation, as to fact, is ſufficiently evident, and the inference therefrom is alſo, perhaps, juſt: yet from my obſervations, in another part, I am led to conclude, that, perhaps, the real life of the blood, in thoſe perſons labouring under inflammation, is not diminiſhed, but only from the violent action of the cauſe inducing inflammation, or a different ſtate of action of thoſe organs which give to the blood its animation, it is rendered almoſt inſenſible to that ſtimulus which formerly freely acted upon it. This argument has alſo analogy for its ſupport; for we often ſee that, by the long-continued application of a ſtimulus to a muſcular fibre, it becomes gradually leſs ſenſible thereto; yet notwithſtanding this, its real life is not diminiſhed.

Having then, as I think, neceſſarily explained thus much, we will next conſider thoſe arguments which have been advanced againſt this poſition; and firſt, thoſe of Dr. Hendy, who at once objects to it in whole, by ſaying, "that from this it would appear to be a mere paſſive kind of life "that the blood enjoys, for it is totally "dependent on the animal powers which "cauſe it to circulate."

But this is certainly no argument; we might as well ſay the ſolids were not alive, becauſe they were dependent for this life upon the blood; for certainly their life is as intimately connected with the blood, as the life of the blood is connected with the ſolids: and in another place I will attempt to ſhew, that both life and ſenſation are, perhaps, more commonly affected through the action of different ſubſtances on the life of the blood than has been commonly ſuppoſed; but, adds he, "it muſt be a "curious kind of life it poſſeſſes, as the "more it is alive, the eaſier it is affected "by a ſtimulus; now it is the reverſe in "every other kind of life; for," ſays he, "the weaker the principle of life is, the "more violently it is affected by ſlight "ſtimuli."

This may certainly be the caſe; yet it is no argument that life does not exiſt in the blood; for thoſe effects which follow the application of ſtimuli to muſcular fibre, when its principle of life is weakened, is, perhaps, more owing to the ſtate of organization than that of life; for as in the blood life exiſts in its moſt ſimple form, and unconnected, at leaſt, with that organization which conſtitutes muſcular fibre, from the ſtate of life at the time being alike in both, ſuppoſing them ſo, yet from the want or difference of organization we thus eaſily account for the different phenomena which reſult from the application of the ſame cauſe to both.

But viewing the ſubject as I have already done, and conſidering that in inflammation the life of the blood is not diminiſhed, only from ſome cauſe rendered more inſenſible to the application of a ſtimulus, a ready ſolution at once is afforded of Dr. Hendy's argument, without having recourſe to any other.

But in addition to theſe arguments of Dr. Hendy, Dr. Adye endeavours to combat the idea by arguments drawn from three ſources; but the two firſt of which evidently reſt on ſo ſlippery a foundation as not to need a ſerious conſideration; for we have already proved, and it is, indeed, evident, no analogy can be drawn betwixt the effects which follow the application of the ſame cauſe, to the blood and living muſcular fibre: the arguments, however, are drawn from this analogy, which, being falſe, are at once refuted: and as to the third, which we propoſe to conſider at more length, I expect it will ſhare the ſame fate; it is: "Si ſanguis ſerius coiens ſuæ a ſanitate "aberrationi eſt referendus, unde citius "cogitur in morbis putridis, quam in in-"flammatoriis; cum in illis, quam in his, "fluida majis afficiantur. Præteria cum "contagio, ſi ab effectibus quos edat judi-"care liceat, vim magnopere ſedantem "habiat, in morbis ab hac originem dicen-"tibus minus irritabilis eſſe debit ſanguis, "ideoque ſerius in coagula abire."

In the firſt part of this argument Dr. Adye aſks why, if the late coagulation of the blood depends on a leſſening of its vital principle, is it ſooner coagulated in putrid than in inflammatory diſeaſes, as in this latter the fluids are leſs affected than in the former.

To this I would anſwer, that although the late coagulation of the blood, in one inſtance, depends on a diminution of its living power, we cannot infer it to be a general rule, and occurring in every caſe; for although we find the blood coagulates later than uſual, in both putrid and inflammatory diſeaſes, we cannot thence conclude that in both the living power of the blood is diminiſhed; on the contrary, as I have already hinted, and will hereafter endeavour to prove, every circumſtance tends to ſhew, that in inflammatory diſorders the vital principal is nothing diminiſhed, only from violent action, (which Mr. Hewſon has ſhewn is a cauſe adequate to explain the phenomena) rendered not ſo ſenſible to the application of a ſtimulus; while in putrid diſorders every circumſtance connected both with their hiſtory and treatment, ſufficiently evince the diminution of life in every function; and, without doubt, a real diminution here occurs of the blood's life, owing to which it coagulates later than uſual.

This view of the matter is even farther confirmed from another circumſtance, and that only a diminution leſs or more of the blood's living principle occurs in putrid diſorders, is its coagulating in ſome caſes ſooner than in the inflammatory; while in other caſes of putrid diſorders the diminution of the blood's life is ſo great, that no coagulation even follows from expoſure.

From all this, then, the evident anſwer to Dr. Adye's queſtion is, that the leſſened diſpoſition of the blood to coagulate in inflammatory diſorders, depends on an inſenſibility of its life to the action of a ſtimulus, ſolely induced by violent action, and without any real diminution of its life; while in the putrid, although the blood appears more affected, and is in reality ſo, yet its leſſened diſpoſition to coagulate depends upon a diminution alone of its living power, and from a greater or leſs diminution of life or vigour the ſtimulus of air produces its effects with more or leſs facility.

To the ſecond part of the argument, that which ſuggeſts a doubt about the action of contagion, I have only in this place to obſerve, that it is more than probable that, in ſuch caſes, contagion has a ſedative power; but what is more uncertain, is the aſcertaining on what this ſedative power acts; to me it appears more than probable,

and

and I will ſtate my reaſons for this hereafter; that its firſt action is on the blood, and that the diſordered ſtate of ſenſation, and the power of motion, are only conſequences reſulting from the action of blood thus changed by contagion; and hence we really find, that blood in theſe diſorders ſhews that its life is diminiſhed: and therefore in anſwer to the queſtion, we ſay, and obſervation ſeems to confirm it, that in proportion to the virulence of the contagion, and conſequent diminution of the living principle, the blood in theſe diſorders has more or leſs tendency to coagulation. And this anſwer remains the ſame, whether we ſuppoſe the blood primarly affected, or that the diſordered ſtate of it is only a conſequence of the morbid alteration in the action of the moving fibre, or nervous ſyſtem induced by the ſpecific ſtimulus of each contagion, and by that alone affecting our fluids.

Having thus then, as briefly as the nature of the ſubject would allow, conſidered thoſe effects which follow the application of a ſtimulus to the living principle of the blood, and ſhewn that it acts thereon in a peculiar manner, but agreeable to thoſe laws which govern living bodies, and having alſo ſeemingly invalidated thoſe arguments which have been brought againſt Mr. Hunter's data, we, without farther reaſoning, proceed to the conſideration of the third ſection of the definition, which is, that it is poſſeſſed of a power by which it reſiſts the action of thoſe cauſes which tend to its deſtruction.

As we have ſaid before, this is a conſtant and invariable effect which reſults from the action of certain agents, on a living principle; living bodies are alone poſſeſſed of it; and hence if we find the blood poſſeſſed of ſuch a power, we muſt irreſiſtibly infer a living power in it, although we were deſtitute of any other argument in ſupport of it. Mr. Hunter, ſenſible of this, brings forward the following argument, founded on fact, and ſtrictly juſt: he ſays, "Blood taken from the arm, in "the moſt intenſe cold which the human body can bear, raiſes the thermometer to the ſame height as blood taken "in the moſt ſultry heat. This (adds he) "is a ſtrong proof of the blood's being "alive, as living bodies alone have the

" power of reſiſting great degrees both of
" heat and cold, and of maintaining, in
" almoſt every ſituation while in health,
" that temperature which we diſtinguiſh
" by the name of animal heat."

The opponents of the opinion, that life exiſts in the blood, allow, ſenſible they cannot do otherwiſe, that this argument is a ſtrong proof that life can reſiſt the action of external cold; but they will not admit that it is any proof this power exiſts in the blood; on the contrary, with a view to its refutation, Dr. Hendy aſſerts the following: "That blood, drawn in ſeveral different circumſtances of the air, in leſs than two hours will be cooled to the degree of the ſurrounding atmoſphere." That this is the caſe from experiment I am fully convinced; but from the ſame ſource alſo I am as fully convinced, that a quantity of ſtarch diſſolved in water, ſo as to make a fluid as thick as blood, and being of the ſame temperature as the blood drawn from its veſſels, in like quantity, and placed under ſimilar circumſtances, comes to the temperature of the air conſiderably ſooner than the blood. From his experiment, however, he infers, "that blood, inde-"pendent of the animal ſyſtem, cannot "retain heat; hence it cannot be alive."

To this concluſion I only remark, that I cannot ſuppoſe any one could think it poſſible that blood, unconnected with the ſource of life, could reſiſt the action of cold, or retain its heat, for a longer time than till that quantity of life attached to it was deſtroyed, which takes place ſooner or later, according to the quantity of life, or the force with which the hurtful agents act. But that it can reſiſt the action either of heat or cold, and that too for a conſiderable time, even although they be applied in no low degree, I am fully convinced from ſimilar experiments as thoſe inſtituted on the egg. But as was formerly hinted, theſe experiments being made upon a ſmall ſcale, I ſhall, inſtead of relating them at full length, merely mention the reſult.

Having immerſed a quantity of blood juſt drawn from the arm of a ſound perſon, in a freezing mixture, till it was frozen, and after having continued in that ſtate for ſome time, I thawed it, and brought it to the ſame temperature as when drawn from the vein; I then immerſed it again into the ſame freezing mixture along with another quantity of the ſame blood directly drawn from the ſame perſon, and in exact proportion; but the one which had been frozen and thawed again, froze many minutes ſooner than that directly from the vein. The reſult of this experiment was as I expected; but I ſhould have diverſified it conſiderably, had not want of materials prevented me, which I much regret. At the ſame time I repeated the ſame experiment on blood contained in its proper veſſel by ligatures, and found the reſult to correſpond, taking into account the little quantity of blood ſubject to the action of the cold.

I alſo found by ſubjecting a quantity of blood to the action of electricity from a large battery, that its power of reſiſting the cold was conſiderably diminiſhed; and in one caſe I thought its diſpoſition to coagulation much diminiſhed, nay entirely deſtroyed.

Theſe experiments, imperfect as they are, ſuggeſt many others, and which the firſt opportunity for procuring materials

I will perform, eſpecially if I can procure any of the American poiſons, or viper's venom, which, I think, poſſeſs the power of immediately deſtroying the blood's life, and which by a freezing proceſs I ſhall be able to aſcertain.

From thoſe experiments it would appear then, that blood, unconnected with the ſource of life, has a power of reſiſting, as other living bodies have, the action of cold as long as this life remains, which fully confirms Mr. Hunter's argument; but without inferring any thing from theſe experiments, Dr. Hendy's objections reſt on ſo ſlippery a foundation, that they can be invalidated by merely turning the argument againſt himſelf.

He ſuppoſes life can only be attached to the ſolids of animals; but this, taking his rule of life along with us, we could not infer, becauſe we find that a muſcle cut out of the body, and unconnected with the ſource of life, does not reſiſt the action of cold, or retain its heat, for a longer time than blood does; it is evident, however, both are alive, and both reſiſt the action of theſe hurtful bodies ſo long as they poſſeſs this life; but being unconnected with the ſource of life, this power of reſiſtance cannot continue for any conſiderable length of time.

Thus, then, Mr. Hunter's argument remains in full force—a ſtrong proof of the blood's vitality.

After which we proceed to the conſideration of another argument of Mr. Hunter's —the laſt and only one which we have not conſidered. A doubt, however, may ariſe, whether it more properly belongs to this head, or to that which follows; but for ſufficiently obvious reaſons we conſider it under this; it is, "blood preſerves life in different "parts of the body. When the nerves "going to a part are tied or cut, the part "becomes paralytic, and loſes all power of "motion, but it does not mortify; but if "the artery be cut, the part dies, and mor-"tification enſues."

Thus, then, it is that blood flowing into a part, although unconnected with the nerous ſyſtem, reſiſts the action of heat and moiſture, and conſequently putrefaction, by which life is continued. Viewing it, then, in this light, and that it is from being poſſeſſed of a power of reſiſtance to thoſe agents that life is preſerved in ſuch ſituations, it naturally falls to be conſidered in this place.

The authenticity of this, as to fact, is ſufficiently evinced by every day's obſervation; yet the induction from the data is endeavoured by Dr. Hendy to be combated; for he only allows this "a proof that "blood is eſſentially neceſſary to the life of a "part; and, if proving any thing, it is, that "nouriſhment is chiefly conveyed by the "arteries, and not by the nerves." Even in this view he thinks it hardly ſatisfactory; "for although (ſays he) the larger "branches of the nerves were cut or tied, "yet the almoſt infinite diviſion of nerves "would lead us to conclude, that a num- "of theſe ſmall branches were ſtill left;" and concludes the whole by affirming, "that "though a part deprived of blood mortifies, "yet the fluid which ſuſtains it may not "be alive itſelf; for (ſays he) the whole "animal body cannot exiſt long, independent of certain ingeſta taken into the "ſtomach;" and adds, "becauſe this is "highly neceſſary to life, we cannot from

“ thence infer that the ingeſta muſt have
“ life.”

To theſe arguments, highly ingenious, and, at firſt ſight, not a little concluſive, I have only to remark, that were not the blood endowed with a living principle as it circulates in its veſſels, it could not, independent of the nerves, preſerve life—I ſay independent; for were theſe ſmall branches, (and this is only a mere ſuppoſition) which Dr. Hendy ſuppoſes to remain uncut, capable of preſerving the life of the part in as high a ſtate as before, which experiment has proved to be the caſe, although deprived of ſenſation, nature has certainly waſted a great deal of nervous energy to no purpoſe. None of Nature's works are, however, in vain; for ſenſation, and the purpoſes thereof, ſhe has alone employed them; and upon their deſtruction ſenſation ceaſes.

Moreover, that nervous influence is the only ſource of life, is irreconcilable with many of the phenomena of nature: for example; it is found by repeated experiments, that upon dividing high up the whole nerves of the lower extremity of a frog, and leaving the arteries uncut, that life can be ſuſtained in that limb, for almoſt any length of time, (without ſenſation) as perfect, and the parts as plump, as when the nerves remained uncut.

To ſhew that this is inexplicable by any other means than ſuppoſing the blood poſſeſſed of a living power, will, I think, be no difficult taſk. We have only to mention, that from many arguments and ſome experiments, it is demonſtrated, and, I believe, generally admitted, that the ſolids of our body, as well as the fluids, are always changing; that is, in the former there takes place a conſtant abſorption from, and as conſtant a depoſition to them by the arteries. This depoſition, phiſiologiſts of our days generally allow, is made by thoſe means we have mentioned, directly from the blood, and what is abſorbed is as evidently poured into the blood: only apply this to the caſe in point, and, beyond a doubt, it demonſtrates the living power of the blood; for from it we cannot but infer, that in no long time, what conſtitute, when the neive was cut, the ſolids of the limb, particularly the muſcular and other ſoft parts, would be entirely abſorbed; and what new matter was depoſited, which we ſee was in proper quantity, from the plumpneſs of the limb, ought to have been dead matter, ſince the nerves were cut, unleſs we ſuppoſe that the blood was alive; which, were it not, mortification, according to this view of the ſubject, would ſoon have followed, and as certainly as if the circulation in the arteries had been ſtopped.

Even ſuppoſing ſome ſmall branches left (for the ſake of argument) uncut, yet it is not poſſible to conceive them adequate to animate a quantity of matter ſo great as to require thoſe large trunks which we ſee ſent to the limb. We even here ſuppoſe the nerves give to the muſcular fibre that life which it poſſeſſeſs; yet in another place I will attempt to ſhew, that ſimple life, if I may be allowed the expreſſion, exiſts independent of nerves; and that the muſcular fibre poſſeſſes ſuch a life, and that too immediately derived from the blood, conjoined only with a peculiar organization, is what I expect to make not a little probable.

Thus, then, have we invalidated thoſe arguments which have been brought againſt the firſt part of our poſition, and made it, I think, ſufficiently evident that blood is not only neceſſary to the life of a part, but that it is even capable of ſuſtaining life in a part unconnected with the nerves; and that this could only take place, we have made evident, from its being poſſeſſed of a living power.

Another argument which naturally occurs here, and which ſeems to render the idea of life in the blood ſtill more probable, if need be, can alſo be drawn from this waſte and ſupply of our ſolids.

What is taken in by the abſorbent veſſels, (but how this is affected I cannot even pretend to ſay) evidently before that, was poſſeſſed of life. Now does its being abſorbed deſtroy this life? or can abſorbent veſſels not take in living matter? But, perhaps, allowing that they can, yet it may be aſſerted, that they only take in thoſe particles of the ſolids when dead, and thus render ſuperfluous the other two queſtions in one point of view.

To me it appears, however, that were the particles of our ſolids only abſorbed when dead, and verging to putridity, all thoſe effects would follow their admiſſion, in proportion to their quantity, as follow the abſorption of evidently dead and putrid particles, as in caſes of gangrene, and diſeaſe would always, in a certain degree, be preſent. Beſides, it is impoſſible to figure to ourſelves how dead ſolids could exiſt, without ſhewing their effects as ſuch; and as impoſſible to ſuppoſe how one particle is deprived of its life, while the reſt poſſeſs theirs. But what end abſorption from our ſolids, and a conſtant depoſition thereto, ſerves, I will not take upon me to ſay; certain I think it is, however, that they are in a living ſtate when abſorbed, and thus we have an evident ſource of vitality in our fluids, unleſs it can be ſhewn lymphatic veſſels, or veſſels in general, have a power of deſtroying this life; but ſo far from this, I truſt, I ſhall make it appear they in general poſſeſs a power of animating the blood.

Thus, then, are we led to the conſideration of the laſt part of Dr. Hendy's argument,

ment, which is, "that becauſe certain ingeſta taken into the ſtomach are highly "neceſſary to life, we cannot infer that "the ingeſta muſt have life."

To this argument, in this place, we will merely content ourſelves with obſerving, that it is evident our animated nature derives its origin from inanimate matter: and this queſtion leads to the enquiry, where or in what place matter is ſo changed, ſo as, beſides being governed by the laws of dead bodies, it acknowledges that of the living. In another place I profeſſedly treat of this ſubject, and therefore wave the enquiry. I may only obſerve, that from two circumſtances it is evident that the matter taken into the ſtomach as ingeſta, has, before it enters the red veſſels, its property ſo changed, that it does not produce the effects of dead matter; otherwiſe we ſhould find that milk, a ſubſtance extremely analogous to chyle, mild and bland, would ſhew no bad effects when immediately injected into our veſſels: but this is far from being the caſe.

The other argument is, the peculiarity of the proceſs of digeſtion, and the well-known wonderful power of the action of veſſels.

From this it is evident, then, that I even view the matter taken into our ſyſtem as animated to a certain degree before it enters it; but this, it is evident, we are not under the neceſſity of proving to invalidate Dr. Hendy's argument. We have only to ſay, and, indeed, it is ſo, that it is a matter of no conſequence to us at preſent what the effects of the ingeſta are, and where the ingeſta are animated, ſince we have, by a multiplicity of argument, and even by the argument to which he oppoſes this reaſoning, demonſtrated a living power in the blood.

Having thus, then, conſidered the three firſt ſections of our definition, and, in my opinion, confirmed them from experiments and obſervation, we are naturally led to a conſideration of the fourth and laſt, which is, that the living principle of the blood is the cauſe of all the actions which it induces. This, it is evident, will be a taſk not a little difficult, as it is involved in much obſcurity; it would lead me to inveſtigate many hypotheſes deſerving attention, either from their own plauſibility, or the high repute of their authors.

This, however, is inconſiſtent with the nature of this eſſay; beſides, it is a taſk to which I am not equal; I muſt, therefore, content myſelf with merely mentioning the defects of ſome of the late, and, I may ſay, generally received, ſyſtems, leaving the greater number to fall by their own inſufficiency, or the arguments of others.

Having, then, already demonſtrated that blood, when extravaſated, is governed by the laws of living matter, we certainly cannot with juſtice infer, that thoſe actions which it induces, when flowing in its proper veſſels, reſult from a cauſe of which by a principle it is diveſted. Thus it is evident that the opinion, though novel and new, is not unreaſonable; on the contrary, a natural and direct induction from its animated nature.

That the blood, then, in our ſyſtem being living, its effects cannot be conſidered as reſulting either from its being paſſive, or as dead matter, I will attempt to ſhew, that by viewing it in ſuch a light, that is, as dead matter, we cannot explain certain functions of the animal body; while a ready and known wonderful power of the action of veſſels.

From this it is evident, then, that I even view the matter taken into our ſyſtem as animated to a certain degree before it enters it; but this, it is evident, we are not under the neceſſity of proving to invalidate Dr. Hendy's argument. We have only to ſay, and, indeed, it is ſo, that it is a matter of no conſequence to us at preſent what the effects of the ingeſta are, and where the ingeſta are animated, ſince we have, by a multiplicity of argument, and even by the argument to which he oppoſes this reaſoning, demonſtrated a living power in the blood.

Having thus, then, conſidered the three firſt ſections of our definition, and, in my opinion, confirmed them from experiments and obſervation, we are naturally led to a conſideration of the fourth and laſt, which is, that the living principle of the blood is the cauſe of all the actions which it induces. This, it is evident, will be a taſk not a little difficult, as it is involved in much obſcurity; it would lead me to inveſtigate many hypotheſes deſerving attention, either from their own plauſibility, or the high repute of their authors.

This, however, is inconſiſtent with the nature of this eſſay; beſides, it is a taſk to which I am not equal; I muſt, therefore, content myſelf with merely mentioning the defects of ſome of the late, and, I may ſay, generally received, ſyſtems, leaving the greater number to fall by their own inſufficiency, or the arguments of others.

Having, then, already demonſtrated that blood, when extravaſated, is governed by the laws of living matter, we certainly cannot with juſtice infer, that thoſe actions which it induces, when flowing in its proper veſſels, reſult from a cauſe of which by a principle it is diveſted. Thus it is evident that the opinion, though novel and new, is not unreaſonable; on the contrary, a natural and direct induction from its animated nature.

That the blood, then, in our ſyſtem being living, its effects cannot be conſidered as reſulting either from its being paſſive, or as dead matter, I will attempt to ſhew, that by viewing it in ſuch a light, that is, as dead matter, we cannot explain certain functions of the animal body; while a ready and even direct ſolution is the conſequence, when we view it poſſeſſed of life, which it evidenly is.

To do this, then, and prove my aſſertions, it is neceſſary to bring forward thoſe effects which the blood ſhows on the living ſolid when extravaſated. Theſe, in a former part, we have ſhewn, not to be the ſame as thoſe which reſult from the application of dead matter, and that no effects are produced but ſuch as reſult from the action of one living power upon another.

We have alſo demonſtrated, that the blood is poſſeſſed of two other characteriſtics peculiar to a living ſtate; a power of reſiſtance, and a principle of ſenſibility. Hence it is not unreaſonable to infer, that when circulating in our ſyſtem it is poſſeſſed of a living power: therefore the only natural and direct induction from theſe *data* is, that thoſe effects which reſult from its action on the living ſolids, independent of quantity and force, muſt be a conſequence of this principle acting thereon; for being deprived of a conſtant property of dead matter, we cannot ſuppoſe thoſe effects which reſult a conſequence of a power of which it is not poſſeſſed.

From theſe arguments alone, then, our poſition appears to me fixed and determined, and no farther proof wanted to the eſtabliſhing thus our fourth and laſt ſection of the definition: but we can go farther, and, in my opinion, demonſtrate the impoſſibility of admitting the blood to be dead, and that it acts chemically on our ſolids—a ſtrong proof, independent of any other, of the blood's vitality.

To prove this, then, we will briefly conſider, at this time, the theory of circulation alone; to explain which many different hypotheſes have been invented. Doctors White, and Monro, junior, however, after invalidating all other hypotheſes, propoſe their own, and which, they think, reſts on a ſure foundation; and which at preſent, as far as I know, is the theory univerſally admitted.

From what I have been able to collect from their works, it appears to me that the doctrine which they inculcate is, that from the irritability of the heart and veſſels, (it does not become me here to enquire from whence this irritability is derived) acted upon by the blood chemically, that is, from it, ſtimulant, ſaline property, conjoined with a certain degree of diſtention, reſult the action of the heart, and the whole phenomena of circulation.

That a certain ſtate of the heart and veſſels is neceſſary to circulation, and which may be termed their irritability, is what muſt be granted; but that this ſtate is acted upon by the blood chemically, that is, as we ſee other ſtimulants act on the ſame power, is that part of the doctrine which, I think, cannot be granted; for were its action ſuch, being dead, (for as ſuch they conſider the blood) the ſame phenomena would reſult which follow the action of other dead matter. The phenomena, however, are different; therefore we cannot but infer a different cauſe.

Let us only conſider what the conſequences are of the application of any one ſtimulus to parts endowed with irritability. The effects are too well known to require much explanation, as every obſerver is ſufficiently informed, that after a ſtimulus has been uſed for a length of time, if we wiſh to induce the ſame effects which it induced at firſt, we muſt increaſe the doſe in proportion to the length of time, or the frequency with which the ſtimulus has been uſed.

Hence we muſt, agreeable to this conſtant and univerſal law reſpecting the action of dead matter on muſcular fibres, infer, that were it from a chemical ſtimulant quality exiſting in the blood that the action of the heart and veſſels was induced, this ſtimulant quality ought always to be on the increaſe, to enable it to produce its former effects. This, however, does not take place, if we may judge from its ſenſible qualities, the only teſt we know; nor is it poſſible to ſuppoſe it could take place, as we neither know the ſource from which it could be derived, nor the mechaniſm by which it could thus be uniformly ſupported.

This reaſoning applies equally to the function of ſecretion, and all others in which blood is actively concerned.

The only inference, then, which now appears to me can be drawn from thoſe *data* which we have thus eſtabliſhed (former inductions being obviouſly erroneous) is, that thoſe actions excited in a healthy ſtate by the blood, if not by quantity or force, are a conſequence of its vital power, ſuppoſing one living power acting on another, perhaps ſomething analogous to the ſtimulus of the will acting on the principle of motion.

Having thus, then, fixed that thoſe natural actions which the blood induces, when independent of force and quantity, are a conſequence of its living power, it would naturally from thence appear, that from a deſtruction or diminution of this power a morbid ſtate of action would be induced.

That this is the caſe, and that we ſhall be thus enabled to explain the actions of many ſubſtances, not hitherto aſcertained, is what I will endeavour to render probable; previous to which, however, I ſhall endeavour to remove ſome objections which I conceive may be advanced againſt the opinion offered: they are ſeemingly ſtrong, and at firſt ſight not a little concluſive; but that they are more imaginary than real, obſervation, I truſt, will demonſtrate.

It may be alledged, that the fæces, urine, and other excreta, which no one can even ſuppoſe poſſeſſed of life, nor have they qualities much more changeable, or any of their ſtimulant properties more increaſed by age than the blood, yet we find that through life, from their action on the moving fibre, the actions which they induce for their own expulſion, are nearly the ſame, and perfectly regular at all ages, provided the power acted upon, and the power which acts thereon, are not different from the healthy ſtate.

Here, then, we have to appearance a ſtrong proof in ſupport of the generally-received opinion; yet to me it appears, that obſervation ſufficiently demonſtrates that thoſe actions which are induced, by either urine or fæces, for their own expulſion, reſult not from any quality of theſe excreta in a natural ſtate, but from quantity alone; for were it quality, we certainly would be conſtantly diſcharging both urine and fæces.

That quality, however, will have effect, is what we daily obſerve in caſes where we wiſh to induce catharſis or diureſis: yet this ſame quality, if continued for a length of time, loſes its effects alſo, ſo that no increaſed action reſults from it, unleſs its doſe be gradually augmented. This, then, is almoſt equal to a demonſtration that thoſe fluids poured into the ſtomach, inteſtines, and bladder, although to the taſte evidently ſtimulant, yet from cuſtom this ſtimulant quality has no effect on the irritability of the inteſtines, &c. Increaſed diſcharge may alſo occur, independent of the ſtimulant quality of the excreta, from the diſpoſition to action being increaſed, owing to which ſtate actions are induced the more or leſs eaſily.

Thus it would appear, then, that the ſtimulus of diſtention alone is that cauſe which induces the action, by which excretion is affected in a natural or healthy ſtate of the body.

To this, however, it becomes us to remove another objection, to appearance not a little ſtrong, and which ſeems to demonſtrate that a certain quality of the fæces, as well as quantity, is neceſſary for the diſcharge in a healthy ſtate. For it is found that in jaundice obſtipatio occurs; and this is explained from the want of the uſual ſtimulus of bile in the alimentary canal. Here, however, I beg to differ from thoſe who give this explanation, which, I believe, is univerſally received, for thoſe reaſons already given concerning the effects of cuſtom, and that explanation which appears to be the natural, is, that the obſtipatio is owing to the leſſened diſpoſition of the moving fibre to contract.

This leſſened or diminiſhed irritability is evident over the whole ſyſtem; it ſeems to be owing to the preſence of bile in the circulating ſyſtem either acting immediately on the principle of motion, or through the medium of its action on the blood; and, perhaps, the diminiſhed ſtate of motion in the inteſtines may be ſomewhat owing to the leſs perfect ſtate of circulation in the ſyſtem of the vena portarum. That other excretions are increaſed, appears to me no argument againſt the diminution of irritability, ſince it is evident theſe may be owing to the increaſe of ſtimulant power. This is evidently the caſe in that by urine; for although the fibre be leſs irritable, yet from bile a new and ſtrong ſtimulus is given ſufficient to counterbalance the leſſened diſpoſition of the moving fibre to contract.

Having thus, then, invalidated all thoſe arguments which appear to me at preſent can be brought againſt the propoſed theory of the blood's circulation, and ſeemingly proved that all the actions which the blood induce, are a conſequence of its principle of life, we next come to ſay a few words concerning morbid actions, induced from a morbid ſtate of the blood.

That ſuch caſes occur we have ſufficient experiments to confirm, particularly thoſe inſtituted by the learned and indefatigable experimenter, the Abbe Fontana, in his Treatiſe on Poiſons, who appears highly ſenſible ſome principle exiſts in the blood with which we are but little acquainted: "but," ſays he*, (ſpeaking of the action of the venom of the viper on a principle in the blood,) "what this new principle "is, and what the organs that ſecrete and "convey it to the veſſels — in this," ſays he, "experiment could alone furniſh me "with ſome light, and conduct me to

---

* Vol. I. p. 329.

" ſome new truth. But," concludes he, " where are the experiments to begin?"

By experiment I think I have demonſtrated a principle in the blood, which is that which produces all its effects, if independent of quantity and force; and by induction then, together with experiment, I will attempt to demonſtrate, that it is on this principle the poiſon acts: but were we to enter into into a full diſcuſſion of this ſubject, it would far exceed the bounds of this eſſay; for connected with it is the diſcuſſion of many intricate and obſcure pathological ſubjects. Perhaps the humoral pathology ſo long diſcarded may yet, for ſeveral reaſons hereafter inferred, be revived. In ſaying ſo, I do not mean to infer that any of the antients' theories concerning the particular diſeaſes exiſting in the humours, will be found juſt; on the contrary, all I wiſh to ſay is, (and we may as well deny the exiſtence of the fluids as deny it) that there exiſts, in not a few caſes, a morbid ſtate of the blood, which, when preſent, is always a cauſe of diſeaſe, and not unfrequently of death itſelf.

That this morbid ſtate of the blood is in many caſes the primary diſeaſe, is what I expect to make not a little probable; and alſo that a morbid ſtate of action in the ſolids, will induce, as a conſequence, a morbid ſtate of the blood.

That the fluids are often in a diſeaſed ſtate, or, at leaſt, a ſtate different from that in health, is manifeſt even from their obvious ſenſible qualities; but that a diſeaſed ſtate thereof may exiſt without any obvious change in theſe qualities, we may neceſſarily infer from our ignorance of what the principle of life conſiſts; yet in ſuch caſes it will commonly be in our power to ſhew its morbid ſtate, from its being no longer governed by thoſe laws which govern blood, evidently endowed with a principle of life, as we have demonſtrated that air acting upon this principle of life by its ſtimulus coagulates the blood. Hence the natural concluſion from this is, that when blood coagulates with difficulty, or not at all, when expoſed to the air, that its principle of life is in what may be called a morbid ſtate; and it is as evident this morbid ſtate may either conſiſt in an inſenſibility of this principle, (unconnected with diminution) to thoſe ſtimuli which formerly acted with facility upon it, on a partial diminution of this principle, or a total diminution of the ſame.

That the firſt ſtate, inſenſibility of the blood to a ſtimulus (unconnected with diminution) may occur, the analogy of other parts acted upon by ſtimuli render ſufficiently probable. This ſtate, when it happens, however, I view merely as ſecondary, and the conſequence of a morbid action, perhaps of thoſe organs which contribute to the blood its living power; and that even from a different ſtate of action of theſe organs, induced by different exciting powers, reſult the different or various degrees of inſenſibility, obſerved in the blood in different ſpecies of inflammatory diſeaſes, where, from ſeveral experiments made by freezing, and ſimilar to thoſe already related, I was led to infer, that only inſenſibility to ſtimulus, without diminution, occurred, while diminution by the ſame experiments was evident in the putrid.

In thoſe caſes in practice our chief attention ought to be directed to counteract the morbid ſtate of action; for, on a removal thereof, in no long time, the effects commonly ceaſe, there being a ceſſation of the cauſe.

That the ſecond ſtate, a diminution of the principle of life in the blood, may occur from cauſes acting immediately ſo as to produce this diminution, we are bound to believe, from many obſervations, and more particularly from this general law, that certain matters which in a given degree can produce the annihilation of life; yet from a deficiency of this quantity, are only adequate to produce a diminution of this life, and which diminution will always be in proportion to the quantity or force with which the hurtful power acts. Hence, perhaps, the different degrees of putrid diſeaſes in which the blood is differently affected.

But a queſtion naturally occurs here. Does the cauſe inducing theſe diſeaſes act primarily on the blood? and do the ſymptoms and conſequences which follow, derive their origin from this ſource?

That this is the caſe, recent experiments, and inductions therefrom, appear to me to render it probable. Many ſubſtances, as the poiſon of the viper, ticunas, &c., which were formerly thought to act on the nerves, experiment, our only ſure guide, has proved to be falſe, and that they exert their virulence on the blood alone: and hence, although contagion has not been made the ſubject of experiment, as it is out of our power, yet, from many circumſtances attending the hiſtory of affections induced by this cauſe, we have great reaſon to believe that its primary action is on the blood, through which the principles of ſenſation and motion are affected; for on the healthy ſtate of the blood, we have ſhewn, depends the healthy ſtate of theſe functions. Circumſtances alſo attending the cure of affections induced by this cauſe, ſeem to render the opinion not a little probable, all of them generally conſiſting in ſuch means as tend to increaſe action and ſupply the want, while even ſome of them, opium, and from analogy may we not ſuppoſe the reſt, Fontana has ſhewn to act immediately on the blood.

Here, however, from the nature of the thing, will venture to ſuggeſt a practice, although it has the appearance of novelty. In many caſes of putrid diſeaſes, as the plague, fevers of the typhoid type, ſcurvy, &c., I would recommend transfuſion, or the abſtraction of a certain degree of the diſeaſed blood, and the inſertion of a quantity of healthy from a perſon in a ſound ſtate. The only objection, or rather hazard, in this practice, is, our ignorance of the proper quantity which can be inſerted, there being, I think, from many other morbid caſes, ſufficient reaſon for believing that there exiſts a certain balance betwixt the powers to be acted upon, and thoſe which act thereon; the powers then, in theſe caſes, are evidently leſſened, conſequently thoſe ſubſtances which act on theſe powers muſt be applied in a leſs degree, otherwiſe from their action there will reſult ſuch an expence of animal powers as would ultimately terminate fatally.

This argument, however, ſtrong as it is, is of no weight in another morbid caſe, depending, as I think, upon a diminution of the vital power, by the retention of that hurtful matter generated by the ſyſtem itſelf, and thrown off by reſpiration; which being ſuſpended from any cauſe whatever, acts ſo upon the principle which ſtimulates the heart, and which I have ſhewn to be life, that it is no longer capable of acting upon the principle of motion in the heart, ſo as to induce its contraction and conſequent propulſion of the blood through the whole ſyſtem, on which active life immediately depends. Here, then, the powers are in an ordinary and healthy ſtate; conſequently all that is wanted is, that the power which ſtimulates ought to be rendered healthy alſo, which could be eaſily done by transfuſion, were we not deprived of the means of withdrawing the diſeaſed blood from the ſyſtem, from the want of action of the veſſels. Hence we are under the neceſſity of increaſing the diſpoſition to action of theſe veſſels by external heat, and, as far as poſſible, promoting reſpiration, and the conſequent ſubſtraction of the virulent matter from the maſs of blood.

Dr. Goodwin, in his excellent eſſay on ſuſpended animation, has clearly pointed it out, that the ceſſation of circulation depends on a want of ſufficient ſtimulus in the blood, without venturing to ſuggeſt what this ſtimulant power is which is rendered inert; elſewhere I have ſhewn, that the only power, independent of quantity, which ſtimulates in the blood, is its life; conſequently if there be a defect of ſtimulant quality, which there evidently is, the principle of life therein muſt be affected.

Dr. Goodwin is farther of opinion, that the principle which changes the colour of the blood black, is alſo the ſame principle which deſtroys circulation; but with diffidence I would ſay, reaſoning from analogy, that the principle which changes the blood black, is not the ſame principle which deſtroys circulation, or, in other words, deſtroys the life of the blood; for by experiment it is found *, that the poiſon of the viper, which evidently, when uſed in proper quantity, entirely deſtroys

---

* Font. on Poiſons, vol. I. p. 384.

the life of the blood, ſo far as to deſtroy animal life, has the faculty of changing the colour of the blood, both of warm and cold animals, black; yet it is only a poiſon to the former, being innocent to the latter.

Having thus, then, in a curſory manner, conſidered a few of thoſe caſes depending, either on an inſenſibility to ſtimulus, without diminution, or on ſo far a diminution of the blood's life as that a cure can be brought about by indirect means, we in the laſt place come to conſider that ſtate either depending on an annihilation, or ſo far deſtruction of the blood's life, that by any ſecondary means we are unable to reſtore life to the animal.

The ingenious Fontana has, I think, ſatisfactorily proved, that the poiſon of the viper, ticunas, toxicodendron, cherry laurel, and ſome other vegetable poiſons, act ſolely on the blood; no doubt ſome may deny the experiments; but by candour I am bound to believe them juſt, till ſome perſon of merit will point out their defects, many of them ſhewing that both ſenſation and the power of motion are only ſecondary affected; for theſe poiſons, when applied to the nerves, ſhew no action thereon: and correſponding with this is the effects of ligature above the parts bit, the circulation being ſtopped, the diſeaſe cauſed by the venom, as its total action is on the blood, ſhould be ſo too. This accordingly Fontana found to occur in the caſes of animals for which he made uſe of it *, which never could happen were not the blood the part on which it acted.

We alſo find that parts amputated, immediately after being bit by a viper, notwithſtanding yet in a ſtate of palpitation, to our ſenſes, ſhew no ſigns of diſeaſe, there ſtill ſubſiſting the degree of heat for a length of time it had before cut out, together with a perfect irritability in its muſcles, which continue to move even for whole minutes †.

From theſe circumſtances briefly ſtated then, we are irreſiſtibly led to the concluſion, that theſe poiſons act upon ſome power in the blood; and from the effects

---

* Fontana, vol. II. p. 48.

† Vol. I. p. 291.

we will endeavour to ſhew what this power is, having previouſly made the induction, that although the irritability, which ſome think is the principle on which theſe poiſons act[*], is diminiſhed, and even deſtroyed, yet this may rather be an effect than a cauſe, and owing entirely to the change induced in the blood, rather than an effect of the venom on the muſcular fibre; for as the ſenſibility is affected equally with the irritability, and as the venom does not act on the nerves, but on the blood, this diminution of ſtrength and ſenſation, and likewiſe the diminution of the irritability itſelf, may depend on the blood. It remains for us, then, to determine what this power is on which the venom of the viper acts.

Fontana, ſenſible, as we have already ſeen, that we muſt admit the preſence of ſome principle, and that too of a ſubtle nature, hazards in ſeveral places[†] conjectures, that this principle is ſecreted by the nerves, and by their termination in the blood veſſels poured thereinto, and circulated along with the blood; and on the deſtruction of which principle by theſe poiſons, he ſuppoſes it more than probable death enſues.

---

[*] Dr. Milman.

[†] Vol. I. p. 331, and vol. II. p. 153.

Elſewhere we ſpeak to this; but be it as it may, as I have ſhewn, that the coagulation of the blood, when drawn, depends upon its vital power being acted upon by the ſtimulus of air; ſo when this power to coagulate is entirely deſtroyed, we muſt ſuppoſe that its living power is deſtroyed.

That its life in this caſe is deſtroyed by the action of theſe poiſons, and not, as in ſome other caſes, only rendered inſenſible to what was formerly a ſtimulus to it, both injection of it into the veſſels, and ſimple mixture with blood drawn, ſufficiently demonſtrate.

In the former caſe, from the action of the viper's poiſon injected into the veins, a conſiderable quantity of the blood is fluid, and deprived of the power of coagulation, with an evident tendency to putrefaction, while another part of it is firm and coagulated: the coagulated part, however, although oppoſite to the opinion of Fontana, I view as entirely depending upon a want of force in the poiſon to deſtroy the whole of the vital principle, yet deſtroys ſo much as to produce death.

This opinion is rendered extremely probable, both from the evident coagulation of the blood in the veſſels of moſt dead animals, and even thoſe who die ſuddenly, but more particularly from what occurs from the action of the ticunas injected into the veins; there the death is more ſudden than from the injection of the viper's poiſon, and alſo the conſequences more remarkable, all the blood is in a fluid ſtate, deprived of the power of coagulation from air, and the diſeaſed ſtate of certain viſcera is far more obvious.

Here, then, we infer from its conſequences, the poiſon of the ticunas more active than the viper's, and conſequently from all the blood being in a fluid ſtate, and deprived of the power of coagulation from expoſure that it deſtroys, even in a very ſmall quantity, all the vitality of the blood.

From the weakneſs of the viper's poiſon, I would alſo explain its want of power in killing cold-blooded animals, and not from any thing peculiar in their blood, only, perhaps, that of being more tenacious of its life; for we find the ticunas, which evidently acts in the ſame way, only with greater force, kills them alſo; and hence from theſe circumſtances we are led to a contrary concluſion from that of Fontana*; and inſtead of ſaying, that the effects which the viper's venom produce on the blood are more clear and decided than thoſe of the ticunas, from its producing coagulation of it: I would ſay that this coagulation is a mark of the weakneſs of the poiſon, and only a conſequence of that poiſon being inadequate to deſtroy the whole of the blood's life; while the fluidity of the blood, its greater tendency to diſſolution, and the more obvious changes in certain viſcera, evidently ſhew the greater virulence of the ticunas, and its capability to deſtroy, in a ſmall quantity, all the blood's life.

Moreover, the opinion which I have ventured to ſuggeſt, of the poiſon of the viper being only deſtructive to the blood's life in a given quantity, is rendered probable, even from the experiments of Fontana himſelf, who, following *Mead*, found that the ſaid poiſon has, when mixed with the blood out of its veſſels, only the property of preventing its coagulation when uſed to a conſiderable extent *.

---

* Vol. II. p. 134.

The latter caſe, that is, the effects of the poiſon on blood drawn from its veſſels, tends alſo much to confirm the idea, both of the mode of action of theſe poiſons, and the power on which they act; for from a ſet of experiments †, inſtituted by Fontana, we find that the poiſon of the viper and ticunas, mixed with the blood, prevent its coagulation, and haſten its putrefaction; which laſt circumſtance, independent of others, is a ſtrong proof that theſe poiſons produce, when uſed in ſufficient quantity, a total annihilation of the blood's life, and have not the effect, as may be ſuppoſed, of only rendering the life of the blood inſenſible to what was formerly a ſtimulus to it.

We have thus, then, briefly ſtated our reaſons for ſuppoſing that theſe poiſons act on the living principle of the blood; owing to the deſtruction of which, as it is the cauſe of all the actions which the blood induces, there is a ceſſation of action, and conſequently inſtant death.

---

* Vol. I. p. 313-377.

† Vol. I. p. 377.

For farther particulars I refer to the Abbe's work itſelf, replete with much uſeful information, both as to the mode of action of theſe poiſons, and thoſe medicines which are thought to affect a cure.

To me it appears, that when only a ſmall quantity of the poiſon is applied, and conſequently only ſlight ſymptoms enſue, nature herſelf is the cure, throwing off that quantity of the blood by different ways, which, by the action of theſe poiſons, was rendered inanimate; perhaps the cure might be ſomewhat accelerated by transfuſion; but in thoſe caſes where the poiſon has been introduced, in ſuch quantity as to deſtroy the whole or greater part of the blood's vital power, nothing that I know, nor do I think any thing can effect a cure; the only probable means would be transfuſion; but here inſurmountable bars are preſented to our view, as we know of no power by which we can empty the veſſels;

and moreover, although we did, and could fill the veſſels with healthy blood, yet ſuch a ſtate of very important organs, as the lungs, is ſo ſuddenly induced as is inconſiſtent with life. In vain then we ſeek for a remedy againſt this diſorder. We may find a preventive, or ſuch a ſubſtance as either prevents its admiſſion into the blood; or although admitted, has the power of ſo involving or deſtroying its venomous quality, that from its introduction no bad effects follow.

To one or other of theſe heads, then, we, without doubt, muſt refer the influence the lunar cauſtic ſhews when mixed with viper's venom, of rendering inert its noxious quality; for no ſuch power would it ſhew as a cure, were once the poiſon allowed to act on the blood.

Thus, then, have we briefly conſidered the different ſections of our definition, and, in my opinion, confirmed them all. Hence can we any longer doubt of the blood's being alive, when we find it governed by fixed and conſtant laws of living matter, the only teſt by which we can judge of the preſence of life. We have found it ſenſible to a ſtimulus, capable of reſiſting thoſe hurtful agents which threaten its deſtruction: and above all, we have found that, by viewing the blood as alive, we can only explain the phenomena of many important functions of the animal body.

The next ſubject of our enquiry will be, to determine from whence the blood derives this life, by which we are led to our fourth and laſt general head.

To determine from whence the blood derives its life, is a taſk no leſs arduous than involved in obſcurity; all we can, perhaps, arrive at, will amount to nothing more than a mere conjecture, and what I offer is as ſuch, rather than the reſult of experiment or actual obſervation. The organs which poſſeſs the power of rendering the blood living, or changing inanimate to animate matter, are to us not certainly known; but from different circumſtances, I think that this change is effected not by any diſtinct organ, I rather view it as the effect of the whole abſorbent and circulating veſſels; but, perhaps, ſome change may alſo take place in certain glandular organs, with the uſe of which we are unacquainted; and this the rather, as we ſhall find the lymphatic glands, which were formerly ſuppoſed to perform analogous changes to theſe organs, poſſeſſed of a conſiderable power of animating the blood.

How theſe organs which we have enumerated effect the change which we have aſcribed to them, or in what this change really conſiſts, we are, perhaps, equally ignorant.

That the firſt, however, in all probability, depends on the action (ſpecific, we may call it) of veſſels, is not improbable, as in other caſes we evidently ſee as wonderful changes effected by them, both in the vegetable and animal kingdom; but in what the other life itſelf conſiſts I cannot even heſitate a conjecture; it ſeems to be the moſt ſimple of life with which the blood is endowed; but whether the reſult of a particular organization, or peculiar modification of matter, or on the preſence of ſome inviſible matter ſecreted by either veſſels or nerves, or ſhews thoſe effects from the preſence of ſome immaterial power, is what I cannot pretend to ſay; perhaps we ſhall never be able to aſcertain it; and, at beſt, more properly a metaphyſical than a phiſiological enquiry. We therefore wave the enquiry, and next, previous to our coming to the main point, as it may be of ſome ſervice, we will endeavour to aſcertain if it reſides in any part of the blood more than others.

In what part of the blood life, as we have defined it, more particularly reſides is a matter of ſome doubt; certain I am, however, from experiment, that it poſſeſſes it in the higheſt degree when all the parts are in their proper proportion, and intimately blended together; but that one or more parts of it may have it in a greater degree than others, is, I think, from experiments made by freezing, rendered ſufficiently probable.

Independent of theſe, I would even conjecture that this power principally reſides in the coagulable lymph; not that I deny it to the other conſtituent parts; for they certainly all of them partake of it in ſome degree; but that it more immediately reſides in the coagulable lymph, is drawn from our finding that many parts of all animals have their veſſels only of ſuch ſize as to admit this and ſerum; in it, or the ſerum, then, or in both, reſides this power, ſince they produce the effect of the whole.

That the ſerum has not life, is what I will not pretend to ſay; but certain am I from experiments by freezing, that it is only in a low degree, and very far inferior to that of the coagulable lymph.

Independent of theſe, however, what led me to adopt it, is from an obſervation of the illuſtrious Haller, who, in endeavouring to render probable the opinion of irritability being independent of ſenſibility, infers, that the former has its ſeat in the glutinous matter, connecting the earthy elements of which the fibres are compoſed; and that this irritability ought to be conſidered as a peculiar property of this glutinous ſubſtance, in like manner as gravity is allowed to be a property of matter in general, although its cauſe cannot be aſſigned*.

It is foreign to my purpoſe to determine, whether or no irritability is independent of ſenſibility; it is enough for my purpoſe to acknowledge, that it appears to me highly probable, from many experiments and obſervations lately brought forward by many ingenious men. Taking it for granted, then, that there exiſts a power in muſcles called irritability, independent and diſtinct from ſenſibility, it remains for me to hazard a conjecture concerning its formation.

---

* Act. Gotting, vol. II. p. 152.

Dr. Haller affirms, and I believe it exiſts in truth, that this irritability reſides in the glutinous part of the muſcle; and that this glutinous part is nothing but the gluten of the blood unchanged, only applied, depoſited, or particularly arranged, by the arteries, appears to me no improbable ſuppoſition. The glutinous matter of both, in a dead ſtate, is exactly the ſame.

So far, then, from ſaying that the irritability of this glutinous matter is a peculiar property of it, as gravity is of matter in general, which, for ſeveral reaſons ſtated by Dr. White *, cannot be admitted, I would ſay, that as we have demonſtrated ſimple life, or life, at leaſt, unconnected

---

* White's works, p. 294.

with any viſible organization, to exiſt in the blood, that from the glutinous part of this blood, applied and arranged by the arteries in a particular manner, it forms the glutinous part of the muſcular fibre; and that the power this fibre has of ſhortening itſelf, when acted upon by a ſtimulus, does not depend on the life it poſſeſſes, being different from that which it was when gluten circulating in the blood, but entirely from a different organization, which is certainly ſufficient to explain all the phenomena.

Dr. White employs the ſame reaſoning, to ſhew how the nerves are not irritable, and which is equally applicable to every different kind of conſtituent part of the animal body. He ſays, "Since muſcles "are the only organs of the body, which "by their peculiar ſtructure are fitted for "motion, it is no wonder the nerves "ſhould have no irritability, ſince the "want of it is only a neceſſary conſe"quence of their make *."

---

* White's works, p. 280.

On the principle we have laid down, we alſo eaſily explain the irritability of many plants; and this, when ſeriouſly conſidered, (which I cannot do at preſent) ſerves to render the opinion about the formation of irritability not a little probable.

Then it is evident I conſider the life of the blood as more immediately reſiding in its gluten. To prove this, I at preſent have contented myſelf with obſervation; yet at a future period actual experiment, I truſt, will confirm it.

That that property of the blood, which we have denominated its life, may reſult from the action of thoſe organs which we have marked out, is an opinion which, from analogy, we can at leaſt render highly probable; after which we will uſe a few additional arguments to ſhew that its life is not ſecreted by the nerves.

Previous to conſidering the change which they induce, we will take a ſhort view of the proceſs inſtituted by nature, for rendering inanimate matter capable of being acted upon by that power which gives it the property of animated matter.

Mr. John Hunter was the firſt who ſatisfactorily pointed out how and by what means this change was induced: he made it appear, that it was from the action of a peculiar fluid poured into the ſtomach.

This opinion future obſervers have ſufficiently confirmed; but what the change is which this fluid, perhaps in conjunction with others, produce, is what we are entirely ignorant of, correſponding with no proceſs with which we are acquainted.

The conjecture I ſhould make on this ſubject, were I allowed, would be, that from the action of this peculiar fluid on, or from its mixture with, inanimate matter, it is thereby rendered a fit pabulum for being acted upon by the lymphatic ſyſtem, in which a farther change is induced, by which it is rendered capable of being farther acted upon by the blood veſſels, in which it is evident a farther and very important change is affected.

This opinion ſeems to be rendered not a little probable, from what we ſee happen in ſeveral particular caſes, many ſubſtances reſiſting the action of this fluid, by which they ſometimes cannot get into the ſyſtem, unleſs from a minute mechanical diviſion of their particles, which, when it happens, always a morbid ſtate of ſome function is induced from their acting as dead matter on the living ſolids: and hence to conclude this part and return, we ſay, that as we conſider blood the pabulum of other ſecretions, ſo do we the reſult of the proceſs of digeſtion, the only fit pabulum for being abſorbed and acted upon by the lymphatic glands and veſſels, the product of which is chyle, and which I view as a ſecretion; and as a proof of this, we need only obſerve the colour of this fluid in a lacteal near its origin, and at its termination, after having paſſed through many glands; there the difference is great, both as to colour, conſiſtence, the effects of air, &c.

Moreover, unleſs we aſcribe to the lymphatic glands a power of changing this matter circulated through them, what uſe ſhall be aſcribed to them: that of Hewſon ſeems now neglected, while the ſame fate, I hope, awaits that opinion which ſuppoſes them a ſet of guards placed, as it were, to hinder the admiſſion of hurtful bodies into our ſyſtem. But it appears to me nature in their formation had no ſuch end in view. It is only with ſome of the hurtful bodies that they perform this office, and then they commonly do more harm than good; for ſuppoſing them for a moment poſſeſſed of this power, what is to become of the matter which they thus ſtop in its progreſs? it is evident from the number of valves that they cannot force it back; and the only way we could ſuppoſe it to be thrown off, would be, from the induction of inflammation and its conſequences; but it is at once evident, the remedy, on many accounts, would be worſe than the diſeaſe; and from theſe occurrences the danger would be increaſed.

For any thing we know to the contrary, then, the lymphatic glands may perform this function. I have been told, that an eminent London anatomiſt views them in this light. This power, which I attribute to theſe glands and veſſels, may appear not a little extraordinary; yet from analogy I think it may be rendered highly probable.

Dr. Hendy has, I think, in his excellent eſſay on glandular ſecretion, clearly pointed it out, that the various changes which the glands induce reſult entirely from the action of the veſſels. It would exceed the limits of this eſſay to even attempt an analyſis of them; beſides, the doctrine is now generally received which renders it the leſs neceſſary. We may only obſerve, that from the analogy of vegetables, from none of the ſecretions being found in the blood, and from the effects of the paſſions on ſecretion, we take it for granted, that it depends on the ſpecific action of veſſels.

To draw our analogy, then, the *data* being fixed, let us remark, that nervous energy, many, not without good reaſon, ſuppoſe to be ſecreted in the brain; even the opponents of life being attached to fluids, may we not ſuppoſe the life of the blood alſo ſecreted by veſſels?

An analogy more applicable, however, may be drawn from what we ſee happen in that ſecretion by the teſticle of the male, which we find has the property when mixed, or applied to that matter ſecreted by the ovaria of the female, of making a complete animal.

Hence, ſince we find that from the ſpecific action of veſſels matters are formed which, when they come in contact in certain ſituations, form a complete animal; may we not with probability conclude, that alſo from another ſpecific action of veſſels reſults that property which we call life in the blood?

That this life, however, is not the reſult of theſe veſſels alone, that is, the lymphatics, is not a little probable from thoſe effects which we ſee follow its admiſſion into what are called the blood veſſels; there it becomes more elaborate, and aſſumes, from a white, a red colour; how theſe red particles are formed, and of what uſe they are in the maſs, we are as yet ignorant. Certain I am, that by experiment it will be found that the blood, in this ſtate, ſhews the greateſt ſigns of life: and hence may we not infer, that thoſe veſſels which circulate it have a power of contributing to the ſupport of this life.

This is not a little probable, not only from thoſe arguments which we have already uſed, but alſo from thoſe wonderful changes which Mr. Hewſon obſerved they had on the blood. To them, then, I think it more than probable we muſt attribute the power of perfecting that which was begun in the abſorbent ſyſtem; which change muſt be previouſly affected.

That this change is neceſſary previous to the entrance of chyle into the veins, is rendered probable from what we ſee happen when we inject fluids as bland, and nearly of the ſame appearance and conſtituent parts as the chyle, even in ſmall quantities, directly into the veins; there they induce the moſt diſtreſſing ſymptoms, terminating in many caſes fatally; which, I think, never would occur, had not the lymphatic ſyſtem the power of inducing on the chyle ſome change, rendering it different from inanimate matter, ſince we ſee no effects follow its admiſſion into the veins.

This mode of explaining the change induced on the blood I thus adopt from the nature of the ſpecific actions of veſſels; but we are ſtill ignorant how they effect this change, and equally ſo with that of the ſpecific action, either in glands in which the product of the action is obvious to our ſenſes, or in general morbid actions, from the application of any contagion. In all, however, we ſee the effect, and our ignorance how the cauſe induces the effect weighs nothing, when we conſider the ſtate of human ſcience.

Moreover I was led to adopt this opinion, from its being, to me, the moſt probable of any which I could fancy. It occurred to me that animation might take place in the lungs, and alſo that it might be a conſequence of a principle ſecreted by the nerves, and poured into the veſſels from their extremities. Innumerable and unſurmountable objections, however, ſoon occurred to theſe hypotheſes; a few of which I ſhall mention, and with this conclude the preſent eſſay.

That in the lungs a change is induced, is ſufficiently obvious, and that, too, highly neceſſary to life; yet, for ſeveral reaſons, I think it not improbable that this change conſiſts in ſomething thrown out, and not taken in. If we thus view reſpiration, it is obvious we rank the lungs among excretory organs, and that by the large extent of ſurface in them ſomething very deſtructive to animal life is thrown off, and which is the cauſe of animals ſo ſoon dying from its ſuppreſſion; but then from a ſuppreſſion of any of the other excretions, death as inevitably enſues as from it; only from the matter thrown off being leſs deſtructive, death is leſs ſudden.

This view I take of the matter, becauſe I cannot ſee any thing like life in air; and that life in the blood only conſiſts in ſimple mixture with a fluid ſecreted by the nerves, is, I think, not more probable; for were not ſome change induced on chyle, previous to its entrance into the veſſels, which could only be effected by its being ſubjected to the action of theſe veſſels, we certainly ſhould never find any danger reſult from the injection of bland fluids into the red veſſels, if ſimple mixture was all that was neceſſary, as they would be ſo immediately mixed with a large maſs of animated matter, that, did not life reſult from the ſpecific action of veſſels, no bad effects would follow. Beſides this, the knife of the anatomiſt can demonſtrate few or no nerves terminating in the blood veſſels; nor is this matter, ſo ſaid to be ſecreted, itſelf more obvious: both of which, at leaſt the former, would certainly occur, were the animation of ſuch a large maſs of matter owing to that cauſe.

Thus I conclude this eſſay, deeming it unneceſſary to apologiſe farther for the many looſe hints which I have thrown out, and the feeble attempts I have made to overthrow theories ſanctioned by great names, and pretty generally received—relying on the candour and liberality of a judicious public, ever ready to treat with lenity the errors of thoſe who even endeavour to extend our knowledge, and to give them that praiſe due to a laudable inclination, although the ſtate of their abilities or opportunities may have been ſuch as to deny them the praiſe of conſiderable progreſs.

The candid public will alſo, I truſt, excuſe me, when I ſeize upon this opportunity of publicly returning my ſincere thanks to Mr. James Pearſon, of Ayr, for the many ſingular favours and ingenious communications with which he has been pleaſed to honour me, and that too with no leſs politeneſs than generoſity.

MVSEVM
BRITAN
NICVM

THE END.